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THE FUTURE OF THE PAST IN THE NORTHEAST

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There was a movie a few years ago in which Paul Newman played a prison farm inmate and Strother Martin played its superintendant. In one scene, which followed an episode of rioting and escape, Martin announced to the assembled prisoners that "what we got here is a failure to communicate." Most moviegoers were amused by this outrageous understatement from the lips of an exponent of bucolic sadism. Northeast archaeology has no notable sadists so far as I know, but our failure to communicate has been in some ways as grotesque as it was for Newman's Cool Hand Luke.

But in many specific instances communication is simply not possible. As Thomas Kuhn explains in his well-known treatise on The Structure of Scientific Revolutions, "when paradigms enter, as they must, into a debate about paradigm choice, their role is necessarily circular. Each group uses its own paradigm to argue in that paradigm's defense" (Kuhn 1970:94). Thus when the adherents of opposing paradigms argue, they often do so with fundamental assumptions hidden, and end up shouting past one another in mutual failure to communicate. The situation is even worse if, as is often the case, the opponents think that they are arguing from a shared set of implicit assumptions, for then each side can only see the other as foolishly illogical. I came face to face with this situation at the Hartford meetings fifteen months ago. I attempted at that time to explain what I see as the new paradigm and provide a few examples of its implications. What I said delighted a few and outraged more than a few. The effect continues because the paper was later published (Snow 1978).

I said what I did in Hartford because I think that archaeology is in the midst of a scientific revolution that goes beyond the rhetoric of those that call themselves "new archaeologists." It began "long before that precious and prissy phrase gained currency," as Glyn Daniel (1976:181) has put it. I don't much like the attitudes of scientific messiahs, and I am pleased that there is no such pretension in the papers delivered here today. Indeed, I think that the current revolution has been brought upon us by events and unforeseen consequences, not the combined will of archaeological intellectuals. So while none of us here can claim the credit for the new paradigm, neither can we be blamed for it by those who prefer the old.

As I said in Hartford, I believe the most potent force in the emergence of the new paradigm has been radiocarbon dating. If one examines the McKern Midwestern Taxonomic System and the later revised system of Willey and Phillips (1958), one sees more than just terminological refinement; one sees a subtle shift in the way in which archaeologists thought about their data. This shift was made possible and then compelled by the accumulation of stratigraphic sequences that changed the relationships between what was assumed, what could be observed and what could be concluded as a result. The effect was an abandonment of the McKern system and general adoption of that proposed by Willey and Phillips. I think that the accumulation of radiocarbon dates over the past quarter century has once again changed those relationships. In the early years of radiocarbon dating, we tended to use it to verify stratigraphic relationships, to provide support for stage sequences, and to anchor horizon styles in time. So long as we stick to the paradigm that is implicit in Willey and Phillips' system, this is a proper use of radiocarbon dates. One assumes that horizon styles exist and that radiocarbon dates from different sites that pertain to a given horizon will distribute normally around a specific date. According to the canons of this paradigm, if a given radiocarbon date does not fall where it was expected to fall then either the temporal position of the horizon must be adjusted or the date itself must be discredited and discarded. This is the set of assumptions and operations underlying that position that there is no convincing evidence for a large time lag in diffusion of early southeastern traits into the Northeast despite apparently anomalous dates ranging from 5100 to 5570 BC for Kirk-like points at Sheep Rock, Harry's Farm and Rockelein. The dates are too young for the Kirk styles if one presumes a flat Kirk horizon in the Eastern Woodlands. These dates support each other, and so far as I know are the only ones we have in good association with Kirk points in the Northeast. Yet the precedent of dates on Kirk points in the Southeast and the acceptance of the concept of horizon style compel the conclusion that the dates from the Northeast are all too young.

For me, however, the inexorable accumulation of tens of thousands of radiocarbon dates has produced an intolerable number of cases such as the one just cited. Perhaps the best example from the Northeast is the inconsistency of the relative and absolute ages of "Laurentian" and "Narrow Point" elements from one part of the region to another. Funk tackles this difficult problem in his major work on Hudson Valley prehistory (Funk 1976:268-276). Although others might prefer to solve

the problem by discarding "bad dates," Funk does not allow himself an easy escape via the old paradigm, and the result is complex discussion that I regard as the most stimulating section of the monograph. Rather than stick to the old paradigm, which he sees as no longer able to support the weight of dates that do not conform to expectations, Funk has implicitly adopted the new paradigm. The new paradigm holds that all radiocarbon dates should be accepted at face value unless they can be clearly rejected for reasons of contamination, poor association, or other technical reasons.

The dates, rather than assumptions about the time-space characteristics of archaeological data, are then allowed to determine contemporaneity or the lack of it. Under this paradigm, such things as horizon styles and cultural stages may be discovered but are not assumed a priori as working concepts. Once again we have a profound change in the relationships between what can be assumed, what can be observed and what can be concluded as a consequence. James Stoltman has implicitly accepted the new paradigm in his recent article on "temporal models in prehistory" (1978). Although I disagree with some of his suggestions, and doubt that the model can be successfully applied in the Northeast, I applaud his forceful argument in favor of using periods, time units that explicitly lack a conceptual implication of stages. I think we can all guess the answer to David Braun's rhetorical question "is the woodland concept really necessary?" "Woodland" still carries some of the conceptual baggage loaded on it by McKern two paradigms back, and although the word may have some continued use as a period denominator, I would not mourn its loss from our special vocabulary.

I want to make it quite clear that in showing how events have altered archaeological epistemology I am not attacking the personalities or impugning the abilities of my colleagues who prefer the old paradigm. The formulae of Newton still serve well at velocities well below the speed of light, and the paradigm outlined by Willey and Phillips still serves many archaeological purposes. But it does not serve all of them, and the going gets rough as distances increase, as Jefferson Chapman and Bill Fitzhugh indicate when they give us their perspectives on the Northeast. Like many others at this conference, I find myself interested not in the problems that can be solved using the old paradigm, but those that it cannot help to solve, for there lies the challenge for archaeology today. We are asking questions that could not be asked twenty years ago. We are bringing conceptual and technical tools to bear upon those problems that did not exist twenty years ago. And we are coming up with answers that have little or no relationship to the purposes of archaeological research as they were defined twenty years ago or more. Little wonder that some archaeologists feel like Alice did when she passed through the looking glass. Little wonder that so many scholars that are supposed to be working in the same discipline accuse each other of doing things that are not archaeology at all. Like physicists some decades ago, I think that we are finding that the world is queerer than we once imagined. But we have one advantage over physicists, and that is that the archaeological world is probably not queerer than we can imagine.

There are other dimensions to the future of archaeology that emerge in other presentations to be heard today. Some have less to do with pragmatic revolution than with a simple shift in what is nowadays considered interesting. I remember the gnawing apprehension I felt as an undergraduate when I discovered that although I was keenly interested in prehistory, I was bored to death by most of what established prehistorians wrote. However, for me at least that has changed. Dena Dincauze has said that a continued strategy of building site-specific sequences and cross dating by means of normative artifact types is no longer an adequate preoccupation for Northeast archaeologists. We will simply not grow or even survive as a discipline if we merely continue to act out the implicit research strategies of pre-radiocarbon archaeology. We need, as Al Dekin will explain, regional research designs if we are to answer the questions I find interesting. We need sober topical studies such as Engelbrecht's approach to prehistoric social organization if we are to break out of the pattern of using the ethnographic record merely as a source for common-sense analogies in implicit archaeological interpretation. And we need the work of Ed Rutsch, John Worrell and others like them to show us all the way out of the traditional narrow definition of Northeast archaeology as equivalent to Northeast aboriginal prehistory.

Each of the following contributions reflects the general proposition that paradigmatic change tends to be forced by the accumulated effects of new data and new techniques, not a deductive tour de force by an archaeological Ayatollah Khomeini. Many regional research programs such as my own in the Lake George region are possible because funding for such efforts is now available through various contract arrangements with public and private agencies. Regional research programs work because we now know how to apply relatively new search, sampling, storage and statistical techniques that were not within our grasp two decades ago. Dincauze and Meyer's (1977) predictive study of prehistoric resources of east-central New England is an example of a project that could not have been launched twenty years ago for lack of funds and which would have failed in any case due to the lack of modern technical resources.

The literature on hunter-gatherers is much more sophisticated today than it was just a few years ago, and it is no accident that much of the best of it has emerged in the archaeological as opposed to ethnological literature. Advances in this area of interest have been made possible not by lofty theoretical breakthroughs but by the accumulation and integration of many new data sets, techniques and working concepts that taken by themselves might seem individually trivial. Yet together they provide us with and force us into a whole new way of thinking about one aspect of archaeology.

Clearly our pool of potential archaeological data has been broadened, and the links between data, analysis and research goals have been made much more explicit. Given this we must break out of the common-sense epistemology of the old paradigm. This is perhaps a more serious affliction for historians than for prehistorians, for prehistorians at least have some anthropological background going for them. Some examples from ethnohistory are useful here. Nigel Davies,

discussing late prehistory in highland Mexico, is willing to "describe the Classic complex of Cholula as an impoverished version of that of Teotihuacan; as such it would hardly have been expected to show greater staying power and to have outlived the latter" (Davies 1977:115). Closer to home, Calvin Martin has dedicated an entire book to the dubious proposition that Indians blamed early historic epidemics on animals and consequently declared war on animal populations (Martin 1978). I have been unable to fathom just how either of these assertions by respected historians can be made to fit the ways in which real human beings behave or the ways in which real cultural systems operate. Both may satisfy the canons of modern western common sense, but that has too little to do with anthropological reality to suit me.

Still another unfortunate use of common sense can be cited from regional archaeological literature. Some years ago I complained in a symposium paper that archaeologists were adopting the concept of biotic province very uncritically and using it in ways that its authors did not intend. Specifically biotic provinces were being used as if they were real rather than abstract units, possessed crisp boundaries, and were permanently fixed in space even though their constituent parts might change over time. I attacked the assumptions hidden in this misuse of concept, particularly in connection with the well-known debate surrounding Early Archaic demography. Dena Dincauze made the very same points in connection with her larger discussion on the evils of common sense at the 1977 Hartford meetings. We have both been privately accused of beating a dead horse by harping on this example, and I was about to resolve never to mention it again when I read a 1978 article by a leading archaeologist. Once again, there it was, big as life, the boundary of the Carolinian biotic province with bifurcate base points distributed south but not north of it. This time the boundary runs across southern Ontario. Clearly this is not the use Dice had in mind for biotic provinces. And just as clearly, it is surely not a productive strategy for understanding prehistoric cultural ecology despite its apparent appeal to common sense. Bear this example in mind when reading Michael Jochim's treatment of postglacial adaptations. And bear the ethnohistorical examples I have in mind when reading Lynn Ceci's contribution. We are about to find out how far we have come from a dependence upon common sense.

I was asked to make this a keynote address, so I have deliberately reserved specifics for use in tomorrow's workshops. I think it much more important to stress in an explicit way the fundamental changes that have been forced upon us all in archaeology, changes that will be detectable in much of what is said here over the next two days. Changes of this sort are not uncommon in scientific disciplines, and in fact archaeology is one of the last of the natural sciences to pass through a metamorphosis that has analogues in both biology and geology. Like those two sciences before us, we are in a process of change that will leave many of our non-professional supporters feeling abandoned and bewildered, but will take us into a more institutionalized service role via public archaeology. Scholars who are the archaeological equivalents of mammalogists, ornithologists and paleontologists will tend to disappear from university campuses (though not necessarily from museums) and will

be replaced by specialists who do not define themselves by the time-space units they study. I once remarked on this disciplinary evolution to the chair of our Department of Biology, noting that what had already transpired in biology was about to happen in archaeology. Not only were archaeologists redefining themselves in ways paralleling the earlier restructuring of biology, but archaeologists were going through the same phase of flirtation with nomothetic deductive theorizing. I was particularly struck by the vision of archaeologists marching to Hempel's drum even as biologists were giving up on him, and had a few misgivings about where we were heading. When I asked him whether he regretted what biology had lost, he said that at times he did, but that biology had gained more than it lost, it had become a discipline. I think that is what is happening to archaeology and that is the common theme of the keynote presentations and workshops of this conference. Archaeology in the Northeast is at last becoming more than an art.

As I said earlier, it is also becoming more than prehistory. What is considered archaeology is much more broadly defined and contains much more topical diversity than was the case 20 years ago. In addition to this expansion of scope, archaeology has taken on a whole new dimension in the form of cultural resource management. The implications of this new dimension have not been fully internalized by many archaeologists, however, and scholars dealing with this subject should direct at least part of their efforts to an exploration of the potential conflicts between the goals of resource management and academic archaeology. The preservationist ethic strictly applied sometimes collides with the demands of academic archaeology. The conflict, it seems to me, could be ameliorated if preservationists could retreat far enough from total preservation and problem-oriented archaeologists could retreat far enough from large-scale excavation to allow the discovery of common ground. Surely we can do a better job of devising sampling strategies that will allow us to both preserve and exploit archaeological resources.

Through cultural resource management, archaeologists have come to serve society at an institutional level even as they have abandoned the more traditional service contacts with amateurs. Although many of us hate to see that old alliance break down, the hard truth is that amateur archaeology has about as much relevance in modern society as amateur dentistry, except in those situations where amateurs receive appropriate training and are clearly subordinate to professionals. I submit that the most appropriate bridge across this widening gap is to be found in stepped up efforts to offer undergraduate and graduate-level training in the context of continuing education. Like geology, biology and some other popular disciplines before us, archaeology is about to abandon one constituency for another; it is a choice that I do not think we have to make. The amateur constituency from which we have drawn support in the past must be challenged to keep up with progress, and those of us in colleges and universities must offer the means for them to do so.

So we find ourselves at this spot in time and space, ready to have a go at building upon the body of knowledge that has been accumulated over the past century, but doomed in the attempt if we fail to recognize the profound shifts that have occurred in archaeological epistemology. This

is not a conference in which an old guard reasserts an outmoded paradigm under the camouflage of a new jargon. This is not a conference that accepts scholarly consensus as a substitute for explicit scientific analysis. And this is not a conference from which we all go home with feelings of security and contentment. We leave stimulated by a new awareness of both our ignorance and the ways in which we can do something about it. We are, as biology was only a few years ago, a discipline in the throes of rebirth. And as Richard Woodbury (1979) said in a recent review "although archaeology may not yet be a science, it is on the right track at least, and 'the best is yet to come!'"

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